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ABSTRACT

In "School Politics Chicago Style," Paul E. Peterson is quick to credit Graham Allison's work. There are major differences between them, however. The authors reach different conclusions about the role of rationality and bargaining because they use different definitions, because of the influence of the dominant mode of thinking at the time they wrote, because of the differing character of the puzzles examined, and because of the questions the analysts wanted to answer and why they wanted to answer them. Three lessons can be learned from efforts to apply models to decision-making. First, alternative models hold out the possibility of a better, if not more complicated, understanding of how decisions are actually made and how programs are actually implemented. Second, models have added measurably to the complexity of conducting policy research. Third, the utility of a model depends in part on the personal judgments of the analyst on what is important and appropriate for study. Models of decision-making, whether frustrating or not, have helped analysts gain a deeper understanding of society and its study, and they are essential for the further development of policy analysis. (Author/IRT)

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MUSINGS ON PAUL PETERSON'S *SCHOOL POLITICS CHICAGO STYLE* AND ON THE UTILITY OF DECISION-MAKING MODELS*

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MUSINGS ON PAUL PETERSON'S *SCHOOL POLITICS CHICAGO STYLE* AND ON THE UTILITY OF DECISION-MAKING MODELS*

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Paul E. Peterson, in School Politics Chicago Style,[1] has written not one, but two books. One book, as the title implies, describes Chicago school politics. In it, we learn about how a big city school board coped with the racial and economic crises of the late sixties. We learn about the school board fights between the reformers and the machine, and we learn about the isolation of the board and the autonomy of its staff. We also learn about those fascinating politicians, Richard J. Daley and "Big Bill" Thompson. And we learn about the shenanigans, delightful with time, of one particularly creative Chicago school board that sold school property to its political pals, only to have the board members end up in jail. For any reader with an interest in Chicago politics or urban school politics in general, Peterson has provided an excellent, if not always easy to follow, piece of research.

Peterson's second book, if you will, is a rather extensive exploration of conceptual models of decision-making. This is followed by the development of his own models, and

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*I am indebted to David Cohen, Alan Doyle, and Charlotte Kuh for their helpful comments on earlier drafts. I have also greatly profited from conversations about models with Richard Elmore.

then their application in the interpretation of several important events in the recent history of the Chicago public schools. It is this feature of Peterson's work that is the focus of this symposium today.

In developing his models, Peterson is quick to acknowledge his intellectual debt to Graham Allison. As you will recall, Allison, in his book, Essence of Decision,[2] developed and applied three different conceptual models to key puzzles about the Cuban Missile Crisis. The net effect of Allison's work was to sensitize analysts to the role played by implicit conceptual frameworks, to open up debate about the use of explicit models in policy analysis, and to call into question much of the contemporary analysis of foreign affairs.

While Peterson is quick to credit Allison for his ground-breaking research, he is equally quick to distinguish his work from Allison's. Peterson does so first, by refining and dissecting Allison's bargaining model. The result is two competing models: one, labeled "pluralist," focuses on bargainers with an interest in re-election and also in compromise among competing group demands; the second, labeled "ideological," focuses on decision-makers who are not primarily motivated by re-election but by their deeply felt beliefs. This distinction is important, Peterson argues, particularly in an urban area like Chicago. Different models

are needed to explain the bargaining behavior of Mayor Daley, as contrasted with the bargaining between the machine and the reformers on the Chicago Board of Education.

A second and critical difference between Allison and Peterson centers on the importance of rationality. As Peterson puts it, "Allison sought to debunk the rational decision-making model; I seek to rehabilitate it."[3] Indeed, Peterson seems prepared to argue that the rational model, for many types of decisions, is not the least but the most useful framework for understanding and predicting policy-making behavior, at least if one is not preoccupied with precision.

Having developed, refined, and amended earlier models, Peterson ends up with four of his own: two varieties of bargaining, a rational model, and an organizational model. He then goes on to apply these models simultaneously to three core issues in the life of Chicago schools in the late sixties: desegregation, political decentralization, and collective bargaining.

It almost goes without saying that Peterson's new book represents a major contribution both to the literature on school policymaking and to the ever-expanding literature on models of decision-making. He has written the most comprehensive account of school politics to date, from a decision-making model perspective. He has demonstrated both the strengths and weaknesses of various models, and has shown how enormously difficult it is to understand policy fully.

Finally, Peterson has dusted off the tarnished remains of rationality and, in his view, restored it to its proper place alongside other models of decision-making. We have a first-rate book. And rationality seems to live again.

At this point, after almost a decade of model building debate, several questions are worth asking about this enterprise and about the different conclusions reached by different authors. For one thing, just how useful is the rational model? For another, just how useful are these models in general, and to whom?

The Utility of the Rational Model

In exploring the utility of the rational model, it is first appropriate to ask why Allison and Peterson seem to differ. Why is it that Allison ended up with little use for rationality and Peterson ended up touting its virtue? I think the answer has little to do with the relative quality of the research; both pieces of work are strikingly impressive. Rather, I think the authors reached different conclusions for at least three reasons that I'd like to discuss now: first, because of different definitions; second, because of the influence of the dominant mode of thinking at the time the two texts were written; and third, because of the differing character of the puzzles examined. Later in the paper, I'll explore a fourth related factor in determining the relative utility of models--what questions the analyst

wants to answer for what purpose.

Let's start with definitions. Peterson argues that Allison's definition of rationality was too narrow, making it easy for hard-headed empiricists to say that rationality was not achieved. If goals are inconsistent or if the "best" alternative is not chosen, Peterson interprets Allison as saying, rationality is not achieved. Peterson, on the other hand, defines the rational model much more broadly. He argues that "rational decisionmaking identifies the limits to the range of alternatives that will be considered, even though it may not determine which alternative is selected within that range."[4] In other words, goals need not be consistent, and "the best" alternative, or presumably even an efficient one, need not be selected (although Peterson is somewhat contradictory on this point).[5] He goes on to say that the model's validity depends on showing three things: "that board members agreed on certain objectives, that reference to these objectives was made during the course of policymaking, and that policy outcomes were consistent with these objectives."[6]

With quite different definitions, it is not surprising that Allison and Peterson reached different conclusions about rationality. In fact, it is relatively easy to show that Peterson's tests of rationality fit some decisions that Allison found inadequately explained by the rational model. Definitions do matter.

But the reasons for differences, I would argue, go beyond definitions and include the differing intellectual climates in which the authors worked. Allison, as you recall, was concerned in 1968 with the overly-rational analysis in foreign affairs. The dominant approach, according to Allison, was to assume that foreign nations acted to maximize predetermined goals. Good analysis, then, was thought to consist of identifying the strategic goals maximized by particular action. Allison's purpose, or at least one of his purposes, was to suggest both the narrowness of this view and the explanatory power of alternative conceptual lenses.

On the other hand, Peterson, writing in 1976, was faced with a different dominant pattern of thought--rationality was on the run. In his view, Allison and his followers had set up a "straw-man," thus undermining the legitimate use of a rational model. In this context, Peterson seems to go out of his way to demonstrate that his broad definition of rationality provides a useful tool in predicting policy outcomes.

Finally, a third reason for differences between the authors may have to do with the differing character of the puzzles studied. Peterson reminds us that sometimes policy boards do in fact have agreed-upon goals, and that these goals can limit the alternatives explored and adopted. We all know that, I think, but in much of my work and presumably in Allison's, common goals either seemed trivial,

quite general, or non-existent. Peterson goes a step further, however, suggesting that in highly constrained environments, common goals are central, specific, and important. He argues that in declining urban areas, like Chicago, the issue is not how to split up a bigger and better pie in a prosperous community, but how to deal with economic and social crises and how to compete with the hinterlands. Faced with these constraining forces, even an ideologically split board, Peterson shows, can rally around the core level goals of maintaining social order, of stabilizing the white population, and of keeping the board's basic authority. And by implication, this suggests that schools in general, now coping with decline, wavering public support, and fiscal austerity, are much more constrained than they were in the past. In this context, Peterson suggests that analysts typically underestimate the degree to which board behavior can be appropriately interpreted as goal-directed. In such contexts, I believe Peterson's argument may well have merit.

So where does all this leave us as to the utility of the rational model? First, it obviously leaves us with a definitional problem--whether or not an organization is acting rationally depends upon what you mean by rational. At this symposium, however, I think it would be fruitless to spend a lot of time dissecting definitions and applying labels. Second, it suggests that if Allison undersold rationality in the context of 1968, it may be that Peterson

is overselling it in the intellectual climate of 1976. I leave it to others, at a later date, to draw a firmer conclusion on that. Finally, it suggests that the utility of the rational model may in part be contingent upon the degree of slack in the system at a given point in history. As slack decreases, does rationality assert itself in the face of increasing adversity? Is there literally less and less room for bargaining? Rationality, then, battered and bruised by battle, may be alive after all, at least in some situations.

The Utility of Models

Now let me turn to the utility of decision-making models in general. At one level, asking whether models are useful is a little like asking whether water is useful. The fact is we need both to exist. Whether everyone is conscious of it or not, we all construct a simplified view of reality in order to deal with its complexity--that's model building. The issue, then, is not whether models ought to be used, but whether they ought to be taken for granted. It is here that books like Allison's and Peterson's are important. Both authors force us to be self-conscious about the fact that what seems to be the policy puzzle is partly determined by the lenses we use to view it. Both force us to be explicit about our assumptions underlying decision-making. And both present several conceptual lenses that can be used in examining policy issues.

At another level, however, it is worth asking whether the current craze with explicit models of decision-making is useful. And craze it is. On this panel alone, besides Peterson and Allison, Bill Boyd has applied models to school decentralization.[7] Ted Marmor has applied them to Medicare.[8] David Mundel has used them in his work. And I have applied models to the internal allocation of resources in state departments of education.[9] And, of course, others have used decision-making models in their research. For example, Halperin has used them in the examination of foreign policy.[10] Steinbruner has developed his own models and applied them to defense policy.[11] And more recently, Elmore has written an important paper contrasting various models, including one drawn from organizational development.[12]

Craze or not, one way to explore the model utility issue is to ask what we have learned from the efforts to apply models to decision-making. I'd like to point to three important lessons.

One thing we have learned, I believe, is that alternative models hold out the possibility of a better, if not more complicated, understanding of how decisions are actually made and how programs are actually implemented. The analyst who is clear about the assumptions underlying various conceptual frameworks is in a good position to identify and ask important research questions that might normally go unasked. In my research on state departments of education, for example,

I used a combination of models to explain why a federal program, Title V of ESEA, appeared to be a failure. The program, designed to stimulate institutional reform through flexible funding, instead resulted in a crisis orientation, marginal changes, and entrenched projects. To deal with this puzzle, a bargaining and organizational process orientation pointed me to important questions about internal competition for funds and about the role of organizational routines in limiting the alternatives examined. This orientation, I believe, led me to a more comprehensive understanding of how complex organizations respond to flexible funding.

In my judgment, models have been particularly helpful in looking back at the '60s and providing a variety of alternative explanations of "what went wrong" in various policy areas. This has provided us with a richer, more complex understanding of how the political system seems to work, and has led to crisper arguments over competing interpretations. Moreover, I suspect we have learned to make somewhat better predictions as a consequence of model building. This seems particularly true when an organizational process model is applied to policies that rely heavily on bureaucracies for their execution. Having said that, however, in my opinion, our capacity to look ahead and predict with much precision remains fairly primitive. Here the existing models seem seriously deficient.

If the models we have been discussing have added to the richness and complexity of our understanding, a second lesson we have learned is that models have also added measurably to the complexity of conducting policy research. Take Peterson's book, for example. In a heroic act, Peterson simultaneously examined three different policy issues from the perspectives offered by four distinct models of decision-making. If the reader's head is left spinning with the complexity of the analysis, one can only imagine the writer's cramp in putting it all together. And one can perhaps better understand why a decade has passed between the beginning of data collection and this symposium today.

Another good illustration of how the explicit use of models complicates research is found in the evaluation of social action programs. Whether evaluators are aware of it or not, various models underlie the standards of comparison used in making judgments about a program's worth. For example, if a rational model is adopted as a norm, one might criticize an agency for not maximizing its goals. But what if implementation was fouled up by organizational routines, should the agency be criticized for acting the way an organizational process model would predict? Let me give you another example from my research on Title V. Applying a rational model as a norm, one might conclude that the program was a failure for not simulating institutional reform. On the other hand, using a bargaining and organizational

process model as a standard, I suggested that this so-called failure might not be a failure at all because the agencies acted the way organizations might typically be expected to act with free money.

In short, different models can lead to quite different assessments of program success, not to mention different recommendations for improvement. Thus, the evaluator who is self-conscious about models quickly discovers that drawing conclusions about program success and appropriate recommendations can become a vastly more complicated enterprise.

Finally, a third important lesson learned is that the process of choosing and applying models is not a straightforward, antiseptic enterprise, based strictly on the objective facts. Rather, the relative utility of a model, as Peterson argues convincingly, depends in part upon who is asking what question for what purpose. And these questions depend upon such things as the analyst's training, experience, professional position, personal values, and notions of how the world operates and how it can be improved. They also depend upon the norms and traditions of the environment in which the analyst works as well as the contemporary intellectual climate, as I suggested earlier. At bottom, then, the relative utility of models in depicting reality depends heavily on the personal judgments of the analyst as to what is important and appropriate.

This point can be illustrated by continuing the above discussion of Title V. In my research, I made a judgment early on that it was important to challenge the assumption that public bureaucracies made internal decisions in a rational and flexible way. From my experience in government, this assumption seemed shaky and I thought it probably was dysfunctional. I also made the judgment, based largely on my experience, that a standard of rationality might be an unfair norm for judging agency success; good programs might be declared failures because of unreasonable expectations. In this context, I emphasized a bargaining and organizational process model in part to explore these particular purposes.

What this example suggests is that in conducting an evaluation, or any analysis for that matter, one is likely to favor a model (and consequently certain interpretations) that reflects one's own peculiar perception of the nature of the problem and one's own particular perspective. Of course, this doesn't mean that one distorts data to prove a point, but it does mean that one's choice of conceptual lenses and one's interpretations are based on a lot more than the simple facts. In short, the process of applying models has forced us to become more self-conscious about the fact that there is no such thing as a neutral, unbiased observer.

Taken together, then, these three lessons leave us with some rather curious results. For one thing, the more

systematic and sophisticated our analysis has become, as illustrated by Peterson's book, the more complicated, diverse and debatable our understanding of social reality; simple stories have been replaced by multiple interpretations of the same policy puzzle. For another, the more self-conscious we become about the nature of reality, the more we realize that the way social scientists report the world has something to do with the facts out there, but it also has a lot to do with the way we perceive the world and also with our particular biases. Third, the more we use multiple models as simplifying devices, the more difficult and time consuming it becomes to conduct a good piece of analysis. Finally, the more sophisticated social science becomes, the more we end up with competing interpretations of the past and with sharper arguments about the appropriate future.

Now whether all this is useful, to go back to our original question, depends upon how one feels about these results (assuming the picture painted is accurate). And I'd argue that different people feel differently. For those individuals who are comfortable with scientific advancements that lead to a richer and more complicated view of society and of how it is analyzed, and for those who are also comfortable with more dispute, and more self-consciousness about the values of social scientists, I think the introduction and application of models is viewed as extremely useful. I count myself among this group, at least most of the time.

On the other hand, for those individuals who hoped that increasing sophistication in social science would lead to more agreement, to quicker analyses, to more objective findings, or even to streamlined advice for policy-makers, I'm afraid the experience with models has been a source of great frustration.[13] Indeed, sometimes I count myself among this group, particularly when asked to provide a reasonable policy-maker with crisp advice.

In any case, models of decision-making, whether frustrating or not, have helped us gain a deeper understanding of society and its study, and they are essential for the further development of policy analysis. These facts cannot be ignored by those who might yearn for the good old days marked by simple analysis of seemingly straightforward stories. For policy researchers, as for Thomas Wolfe, one can't go home again.

FOOTNOTES

1. Paul E. Peterson, School Politics Chicago Style. Chicago: The University of Chicago Press, 1976.

2. Graham T. Allison, Essence of Decision: Explaining the Cuban Missile Crisis. Boston: Little, Brown, 1971. The ideas contained in this book were first presented by Allison in 1968 at a meeting of American Political Science Association.

3. Peterson, op. cit., p. 128.

4. Ibid., p. 131.

5. Contrast the quote in the text with one a page earlier--"a rational actor selects from the alternatives available to him the one that is most suited for achieving whatever goals (rational or not) the actor has in mind" (emphasis added). Presumably Peterson backs off this definition after discussing some of the problems with it, but the text is not crystal clear on this point.

6. Ibid., p. 134.

7. See William L. Boyd and David W. O'Shea, eds., "Perspectives on the Decentralization of Urban School Districts," Education and Urban Society. Vol. VII, No. 4, August, 1975.

8. Theodore R. Marmor, The Politics of Medicare. Chicago: Aldine Publishing Co., 1973.

9. Jerome T. Murphy. State Education Agencies and Discretionary Funds. Lexington, Mass.: D.C. Heath, 1974.

10. Morton H. Halperin, Bureaucratic Politics and Foreign Policy. Washington, D.C.: The Brookings Institution, 1974.

11. John D. Steinbruner, The Cybernetic Theory of Decision. Princeton, N.J.: Princeton University Press, 1974.

12. Richard F. Elmore, "Organizational Models of Social Program Implementation," University of Washington, Xeroxed discussion draft, n.d.

13. For a provocative elaboration of the argument that better social science leads both to greater utility and greater frustration, see David K. Cohen and Janet A. Weiss, "Social Science and Social Policy: Schools and Race," Washington, D.C.: The National Institute of Education, n.d.